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Correlational Field Methodology in the Study of Stress

The following chapter is an attempt to accomplish two interrelated goals: (a) to summarize methodological and practical issues that are central to designing and interpreting correlational field research on the relationships between stress, health, and behavior; and (b) to provide a description of the research designs, procedures, and statistical analyses used in the Los Angeles Noise Project. Our intent in raising methodological and practical issues is to present a context for viewing the noise project and to provide those not intimately familiar with correlation field research with the tools necessary to evaluate this project and other studies. We hope this section will also be used as an outline for the design of future studies. The purpose of the description of the noise project is to provide an overview of the methodological strategies used in these studies and to aid in the interpretion of noise project data presented throughout this volume.

Designing Naturalistic Studies of Environmental Stress

This section provides an introduction to correlational field research for the uninitiated and hopefully some insight for the initiated as well. It is targeted at behavioral scientists whose methodological expertise is grounded in the laboratory experiment and graduate and advanced undergraduate students who are interested in understanding how one designs and interprets correlational field studies.

In an *experimental* study, investigators use two techniques that allow them to eliminate alternative causal explanations for their results. First, subjects are randomly assigned to conditions. Assuming a large enough sample of subjects is used, random assignment assures the investigator that differences between experimental conditions are not due to differences between groups on any individual characteristic. In other words, random assignment is equivalent to matching experimental groups on *all possible individual difference factors*. Second, other than the experimental (independent) variable, all characteristics of the social and physical environments are held constant across conditions. For example, in an experimental study of the effects of noise on performance, the quiet and noise groups work in the same room, at the same temperature, with the same lighting, same instructions, same experimenter, and so forth. Hence, differences between conditions on the outcome (dependent) variable cannot be attributed to any environmental factor other than the (independent) variable under consideration. *Neither of these tools is available when studying naturally occurring phenomena.* There are, however, correlational research techniques that provide some control over individual and environmental factors. Later, we discuss these techniques in the context of the study of the relationship between exposure to environmental stressors and health and behavior.

There are a variety of correlational research designs that can be employed in the study of environmental stressors in naturalistic settings. These designs vary in the number of alternative causal explanations that remain when the study is completed as well as in their monetary costs, practical difficulties, and the amount of time and effort needed to conduct the study. They also vary in terms of the amount of information they provide in regard to the effects of stressor exposure. To no one's surprise, the costlier, more difficult, and more time-consuming research designs are the one's that typically provide the most information. In the next few pages, we describe a number of design alternatives and discuss the costs and benefits of each. In each case, we use the study of the effects of noise on health to exemplify the implications of the design under consideration. Following this discussion, we present a detailed description of the design and methodology employed in the Los Angeles Noise Project studies.

Individual versus Aggregate Data

The first decision made in designing a correlational field study is the level of analysis. Correlational studies of stressor exposure can be conducted at an individual or aggregate level. In studies on an individual level, measurements are obtained from a sample of individuals. For example, in the longitudinal study of the Los Angeles Noise Project, 262 children *each* completed a number of questionnaires, a variety of tasks, and had their blood pressures taken. In an aggregate-level study, groups are used as the unit of analysis instead of individuals; the means, medians, or rates of the group are used instead of individuals' scores. Hence analyses deal with samples of groups rather than samples of individuals. One might compare, for example, the death rate of census tracts around a noisy airport or noisy industrial area to a sample of census tracts that are similar demographically but are not located in noise-impacted communities. One problem with aggregate analysis is that the variable under study, for example, noise exposure, is often not equally distributed across the entire geographic area that defines the aggregate. In our example, an area of a census tract adjacent to the airport may have ambient noise levels that are considerably higher than areas of the same tract that are further away from the airport. Because measurements, like rates of illness and mortality, are aggregated over the entire geographic area, it cannot be determined to what degree such rates in experimental census tracts are influenced by those exposed to the factor under consideration and to what degree they are influenced by others living in the same geographic area but not so exposed. Aggregate-level analyses also pre-

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clude estimates of variability in individual exposures due to daily travel habits, occupational conditions, and the like. For example, one might live in a quiet census tract but spend 8 hours a day in a noisy factory and commute on a busy expressway. As we will show in Chapter 6, interactions of home and school noise levels are important for some measures of children's health and behavior.

Another limitation of aggregate data is that they do not allow examination of the influence of dispositional or attitudinal differences on the impact of the stressor or examination of the role of psychological mediators in stressor effects. For example, in the case of community noise exposure, we know from survey data that attitudes toward the source of the noise, for example, its purpose and whether it is viewed as necessary, are important determinants of annoyance. Moreover, there is reason to think that noise-induced effects on health and well-being are mediated by anger about the noise and its source (Cohen & Weinstein, 1982). Such hypotheses cannot, however, be addressed in aggregate data sets.

As noted, before, aggregate analysis is usually considered less desirable than individual-level analysis but is sometimes employed because it is a less costly method. There are cases, however, when aggregate-level analysis is preferred to individual level. These are situations where the effect of a risk factor is on a group process. Consider, for example, the effect of noise on the performance of something learned in the classroom. This effect may occur because of disruption of the classroom (group) process such as interference in teacher–student communication, student–student communication, or changes in teaching style. Because the impact of the risk factor is presumed to be on the group rather than at an individual level, an aggregate data point (classroom) is used as the unit of analysis rather than individual students. The analysis of school achievement data from the Los Angeles Noise Project employs an aggregate analysis of this type.

Cross-Sectional versus Longitudinal Designs

Cross-Sectional Designs. A cross-sectional study is one in which predictor and outcome variables are all assessed at the same point (a cross-section) in time.[1] Differences between experimental and control groups in cross-sectional studies can be attributed to any of three alternative explanations. Assume, for example, that workers exposed to noise have higher blood pressure levels than a group not so exposed. One possibility is that their increased blood pressure is caused by the noise exposure. A second possibility is that persons with high blood pressure levels tend to choose or tend to be assigned to noisy jobs. A final possibility is that some *third factor* on which noise and quiet groups differ may actually be responsible for both job assignment and blood pressure levels. Examples of individual-level third factors on which noise and quiet groups may differ include education, race, age, seniority with company, and job difficulty. Environmental third factors on which noise and

[1]We use the terms *predictor* and *outcome variables* to refer to a *hypothesized* causal chain. As we will discuss later, causal inferences are not possible in cross-sectional work.

quiet groups may differ include job-related exposure to air pollution, vibration, and heat. These factors tend to covary with noise exposure and could be the actual cause of elevated blood pressure. Select third factor explanations can be eliminated if the experimental and control groups are closely matched on the factor in question. For example, if noise and quiet groups have equal levels of education, blood pressure differences between groups cannot be attributed to educational level. However, even if the investigator matches noise and quiet control groups on several key factors, it is always possible that some unknown variable, not measured or controlled for, is responsible for both job assignment and blood pressure level.

Longitudinal Designs. In a longitudinal design, data are collected from the same individuals at more than one point in time. In many cases both the predictor and outcomes are assessed at each measurement point, but it is not uncommon for the predictor to be assessed only initially and for the outcomes to be measured at each subsequent measurement point. Longitudinal designs allow the investigator to view *changes* in one or more variables in relationship to one or more other variables. Data from longitudinal studies are generally subject to the same three categories of alternative explanations that apply to cross-sectional studies.

There is one form of longitudinal study, the *prospective* study, that allows the elimination of one category of explanation—the possibility that the predictor is caused by the outcome. A prospective study is oriented toward the future effects of the predictor(s) under consideration. The defining characteristic of a prospective study is that it assesses the relationship between a predictor or predictors at one point in time and an outcome or outcomes at a later point. For example, one could look at whether working in a noise-impacted or quiet section of a factory now is related to the incidence of hypertension 2 years from now. In order to eliminate the possibility that the outcome causes the predictor, one must also equate the experimental and control groups on the outcome variable (e.g., blood pressure) at the initial testing. If the experimental and control groups are identical in terms of the outcome variable at the start of the study, any relationship between the predictor as assessed initially and outcomes assessed at later testings cannot be due to the outcomes causing the predictor. For convenience, we will call a prospective study that equates the experimental and control groups on the outcome variable at the onset of the study a "true" prospective study. In our example, a true prospective study would eliminate the possibility that initial blood pressure level was responsible for assignment to noise or quiet job. Groups can be equated either by restricting the sample to those who are similar at the beginning of the study (e.g., including only those with low blood pressure levels) or by using statistical procedures, such as partial correlation and multiple regression, to equate mathematically the groups. Hence, the true prospective study functionally matches the experimental and control groups on the outcome under consideration and then looks at changes in the outcome over time as a function of the initial value of the predictor variable (see the discussion by Kasl, 1983, on additional complexities and limitations of prospective designs in the study of stress).

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Although desirable, prospective analysis of longitudinal data is not always appropriate. An underlying assumption of such an analysis is that the predictor variable remains relatively stable over the period of prediction. For example, if noise exposure is the predictor variable, the prospective[2] analysis assumes that persons are exposed to a specified range of noise from the original point of prediction until the final point of measurement. If, for example, noise levels varied randomly for different people in the sample, the probability of finding changes in health or behavior associated with *initial* level of exposure would be small. The stability assumption is not usually a problem in studying exposure to environmental stress but can be a serious problem when predicting changes in behavior or health from unstable psychological variables like emotional states. It is advisable, however, when doing prospective analyses to check on the stability of the predictor across the period of prediction.

Although longitudinal studies provide information not available from cross-sectional work, they are more difficult to execute. One of the major difficulties is getting data on the *entire* sample at each measurement point. People move, become ill, change their minds about participating, have changes in their schedules, and the like, that make it difficult to get information on the same sample at several points in time. The longer the duration of the study, the greater this problem.

Subject attrition is an especially difficult problem if leaving the study is related to outcome variables. For example, what could one conclude if people with high blood pressure dropped out of a longitudinal study on the effects of noise more often than those with low blood pressure. If blood pressure levels in one group (e.g., persons living in noisy neighborhoods) are higher than in another (e.g., equivalent persons living in quiet neighborhoods), the attrition bias will lead to the groups looking more similar than they actually are.

Attrition biases can also differ across groups. For example, in the longitudinal noise project study, persons in the noise group with high blood pressure were more likely to drop out of the study than those with low pressure. However, in the quiet group, those dropping out and those remaining in the study did not differ in blood pressure levels. A bias of this sort can make groups look either more similar than they actually are (as in the example given before) or make them look different when they actually are not. This occurs because an attrition bias in one group but not the other results in an increase or decrease (depending on the nature of the bias) in one group mean relative to the other. In order to determine whether attrition biases were acting in the longitudinal noise project study, we used a regression procedure to determine whether attrition itself, or the interaction of attrition and the noise-quiet condition were related to any of the outcome variables as measured at Time 1. A more detailed description of this method is provided later.

A new procedure (not employed in our analyses) is also available for correcting

[2]The exception to this rule is when one is testing the hypothesis that a very short exposure to the risk factor is sufficient to put a person under long-term risk.

such biases in attrition when they occur (Berk, 1983; Heckman, 1979). In short, this procedure creates a model to predict who drops out of the study. It then uses this model to assign weights to the data of remaining subjects based on whether they are like other remainers or like those who dropped out. The utility of this analysis depends on the ability to predict attrition from variables that have been collected in the study.

The best way of dealing with attrition problems, however, is avoiding them. In any longitudinal study, one should use all available means to find and collect measurements from as much of the sample as possible at each measurement point. A procedure employed in many longitudinal studies is to ask each respondent for the name and address of a person who will always know where he or she lives. This information allows the investigator to continue to collect data from people who move. Strategies that are useful in keeping people involved in a longitudinal study include (a) gaining a written or verbal commitment from them at the beginning of the study; (b) assigning a specific interviewer or tester to each respondent so that each respondent establishes a relationship with someone connected to the study; (c) increasing incentives (pay) over the course of the study and providing a bonus for those who follow through to completion. The most important thing is that all research personnel treat respondents as intelligent human beings whose participation in the study is a personal favor (even if they are being paid).

Basic Requirements for Sound Field Research

The previous discussion describes two common field research designs and their potential strengths and limitations. Proper implementation of any design requires an understanding of the phenomenon under study and a resulting sensitivity to possible alternative explanations. In other words, all studies using the same generic design are not equal. For example, two cross-sectional studies of the same phenomenon can be quite different in regard to the kinds of inferences one can make from the results.

Later, we list a number of criteria for judging (and designing) field correlational studies. In creating this list, we have borrowed from a personal communication with Tom Wills outlining the criteria for sound epidemiological work. The impact of psychosocial and environmental factors on health is the balywick of epidemiologists. The methodological issues they face are the same as those that confront psychologists or other social scientists interested in the relationships between stress and behavior in human populations.

In our description of the criteria, we employ the epidemiological term *risk factor* to refer to any factor that might result in an increased probability of developing diseases, behavioral disorders, or other forms of behavioral or physiological disruption. For example, in the case of the noise project, exposure to intense noise levels is the risk factor under consideration.

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Criteria for Judging Field Correlational Studies

Our discussion focuses on five criteria that are especially relevant to interpreting field studies. These include (a) inclusion of an experimental (or case) group exposed to the risk factor and a nonexposed control group; (b) respondents are unaware of the hypotheses of the research; (c) careful verification of exposure to the risk factor; (d) data are collected from the total population at risk or a representative sample from that population; and (e) the risk factor outcome relationship is documented in laboratory studies.

1. Data are collected from an experimental (or case) group, which is known to be exposed to the risk factor and from a control group not exposed to the risk factor. The experimental and control groups should be comparable on factors that would be expected to be related to the outcome variable under consideration. For example, consider studying the mortality rate of those living in communities impacted by aircraft noise. Because age, race, and socioeconomic status are associated with death rates, it is essential that the noise and quiet control groups are adequately matched on these factors. When matching is not possible, there are a number of statistical techniques that allow artificial control of extraneous variables. These techniques vary from stratification of the sample on the variable under consideration, for example, analyzing the data separately for persons less than 20 years old, 20 to 40, and so forth, to the use of partial correlation and multiple regression. These techniques can only be used, however, when the data on the variable in question are available. Hence careful consideration of potential alternative explanations for data must occur in the planning stage of the study. Factors that are often important to consider as possible candidates for "controlled" variables in studies of the relationship between stress, behavior, and health include age, sex, race, income, and education. However, the factors controlled for in any particular study depend on the outcome variable(s) under consideration. This is where an understanding of the phenomenon under study and sensitivity to possible alternative explanations are crucial.

2. Aside from the essential requirements of informed consent, respondents should be unaware of the hypotheses of the research. This is particularly important in research involving psychological variables, where respondents may be motivated to respond in such a way as to confirm what they perceive to be the investigator's hypotheses or to support a result that is consistent with their own political values. For situations in which it is probable that respondents are aware of the purposes of the research—which generally occurs when there is strong and generalized community sentiment about an issue—it is advisable to guard against response biases to the greatest possible extent. Such precautions include careful questionnaire construction, with multiple items and dimensions, imbedding critical questions in a more general context, use of nonobvious or unobtrusive measures, and use of physiological measures (cf. Webb, Campbell, Schwartz, & Sechrest, 1966). Optimally, one would use several types of measures and examine the degree to which the results converge.

Community noise surveys, where respondents are questioned about how annoyed they are with aircraft, traffic, or industrial noise, provide a prime example of this potential bias. In order to emphasize their concern with noise, respondents will often report that their level of annoyance is more extreme than it actually is. Commonly, this is prevented by presenting questions in a context that hides the purpose of the study, for example, imbedding noise questions in surveys about neighborhood satisfaction. One can also avoid leading the respondents by allowing them to raise the noise issue themselves if they think it is important. This can be done by starting interviews with open-ended questions in which respondents are asked to list the annoying aspect of their neighborhood. In the noise project, we collected multiple measures including ones that were not obviously concerned with noise, ones imbedded in larger contexts, and a physiological measure.

3. There should be careful verification of exposure to the risk factor. Probable dose should be determined if possible. *Dose* refers to a quantitative measurement that combines intensity of the risk factor (e.g., sound level, amount of CO in the air or level of density) with the duration of exposure. Dose-response relationships are a particularly valuable form of evidence because they can be used to determine the level of exposure that is necessary to put persons under risk. For example, the Occupational Safety Hazards Administration's industrial noise-level regulations are based on evidence for the dose-response relationship between sound level and hearing loss. Workers are allowed exposure to 90 dB(A) sound for 8 hours, 5 days a week. However, if they are exposed to noise at levels of 95 dB(A), they are allowed only 4 hours of exposure, and if they are exposed to noise levels of 100 dB(A), they are allowed only 2 hours exposure per day.

It is worth raising two methodological issues that are important for the establishment of accurate dose–response curves. First, as noted before, careful attention to exposure outside of the research cite, for example, measurement of daily habits, occupational exposure, and the like, can be important. Second, it is preferable that research designs include a variety of exposure levels. A wide range of values is important because some environmental agents may have nonlinear relationships to health and behavior. For example, many pollutants need to reach a certain threshold before aversive effects are noted.

4. The criteria discussed so far address the internal validity of a study—whether the research was done competently enough to allow any conclusions about the relationship between the variables under study in that sample. Assuming internal validity, it is also important to collect data in a manner that allows external validity—the ability to generalize one's results to the target population. Data should be collected from either the *total population at risk* or a *representative (usually random) sample* from that population. Self-selection by respondents into the study is not appropriate. Representative sampling allows one to generalize from the sample to the population that it represents. One way of estimating the probability that a particular sample is representative is the percentage of the target sample who decline participation. The greater the number who decline, the greater the probability of bias. Although few field studies have participation rates over 90%, it is a

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reasonable guideline to express serious concern if the participation rate drops below 80% to 85%. If possible, it is desirable to compare nonparticipants with participants on demographic variables used to describe the sample. Optimally, the nonparticipants will not differ from the participants.

5. It is desirable if the presumed risk factor outcome relationship can be documented in laboratory studies. As discussed earlier, laboratory experiments can be used to establish a causal link between a risk factor and the outcome under consideration. Converging evidence from both laboratory experiments and field studies is compelling, even if the designs of the field studies are less than ideal. In cases where human laboratory experiments are not feasible (particularly if the disease or behavior under study has a long developmental course), or it would be unethical to expose humans to the risk factor under consideration, studies of infrahuman species are invaluable.

There are other methodological and statistical criteria that should be applied in designing and evaluating field research. These include the importance of using psychometrically valid instruments that are truely measuring the concept under consideration (construct validity), using statistical analyses appropriate to the data (statistical validity) (cf. Cook & Campbell, 1979), and of replicating results across samples and measures. Because these criteria are as important in laboratory experimentation as in field correlational studies, we will not elaborate on them here.

To the extent that a particular study meets Requirements 1 to 4 (as well as having adequate construct and statistical validity), reasonably sound conclusions usually can be drawn from the data. Violation of any requirement demands serious consideration of its implications for interpreting the results. Violation of more than one requirement is often regarded as sufficient to invalidate the study in terms of contemporary scientific standards. To the degree that a *set* of studies reaches the same conclusions and meets Requirements 1 to 5, it is possible to make general inferences about the relationship between the risk factor and outcome.

Practical Considerations in Field Research Design

Methodological criteria are not the only determinants of how a field study is designed. Study design is also affected by practical considerations. Primary among these considerations is obtaining cooperation to carry out the study. There are two essential design issues involved in obtaining and maintaining the cooperation of a sample. The first concern is limiting the intrusiveness of the study procedures on the lives of the subjects. Important areas in this regard are the amount of time required of subjects and the degree to which their privacy is violated. The second concern is convincing those who make the decision regarding access to the sample that the study can provide them or individual respondents with some valuable information or other benefit.

Some critical judgment is required in determining how much time will be required of each respondent and what will be done during that time. An estimate of the maximum amount of time one can ask for should be made in light of the nature of the subject population and the nature of the situation. For example, in the noise project, we were concerned with the attentional span of the children and minimizing the amount of class time a child missed and the number of intrusions in any particular classroom. In the case of parent questionnaires, we wanted to ask only the essential questions so that the time commitment would be minimal and participation would be maximal. Time limits imply limits in the amount of information that can be collected from a respondent. A good guideline for choosing measures is to give the highest priority to those questions and tasks that provide you with information necessary for rigorous methodological control (e.g., education, age, gender) and the information essential for testing the theoretical propositions or practical questions that are being posed. There is always a tendency to add an extra measure or two because the study is set up, subjects are available, and so forth. When considering adding tasks, one should remember that additional task time increases the probability of boredom, inattention, response bias, and attrition. In short, increased subject time often constitutes a threat to study validity.

Questions perceived as intimate or violating one's privacy pose similar threats. Responses to these questions are often invalid, and asking such questions can detrimentally affect respondent cooperation. If not essential, such questions should not be asked. If necessary, the least intrusive form of the question should be chosen. For example, in the noise project we felt it was necessary to get some valid indication of social class of the children's parents. Although knowledge of parent income would have increased the validity of our estimate, we felt that such a question would be viewed as inappropriate by some respondents. Hence, we used two less obtrusive questions—years of education and number of children in the family.

Although investigators usually view the theoretical or practical questions that they are posing as more than justifying their intrusion on the lives of respondents, the respondents and those who control access to respondents may find such justification irrelevant and/or inadequate. Before entering negotiations with a group of potential subjects, it is essential to seriously consider the possible benefits of the project for the participants. If existing measures do not provide such benefits, it is worth considering adding measures that provide potentially beneficial information. In the case of the noise project, we were able to offer both the school and parents information on hearing loss and blood pressure levels in cases where it seemed advisable for a child to be examined by a physician.

In sum, field researchers must be sensitive to the feelings, attitudes, and needs of their respondents as well as to the methodological criteria discussed earlier. Often, such sensitivity requires compromises in study design. In most cases, if the investigator carefully weighs the costs and benefits of potential compromises, a well-controlled study can be carried out. There are cases, however, where the necessary compromises will seriously damage the purpose or methodological validity of the study. These are cases where the investigator needs to seriously consider whether the study is worthwhile.